

Mather, California.  
July 23, 1950.

Dr. K. B. Newcombe,  
Atomic Energy Project,  
Biology Division,  
Chalk River, Ont., Canada.

Dear Howard:

Thank you for sending the MS on anomalous segregations. Unfortunately, I have been en route for the past month, and so did not receive your messages promptly, nor have I been able to give them as detailed consideration as they deserve. However, rather than provoke any further delay, I am writing my immediate impressions.

There is no decisive criticism that I can offer. The data are in agreement with my own, and with those collected by L. L. Cavalli (Cambridge) and by Gordon Allen (155 Corona Ave., Pelham 65, N.Y.) Your conclusions are clearly presented and are in agreement with your data as far as they go. Although I have myself preferred to postpone the publication of such data until the genetics of the persistent diploids is clarified, it does not follow that you should withhold publication, especially if for any reason you will not be pursuing this line of work further. The main critical attitude that is worth mentioning is perhaps that the concept of linearity must still be defended, and that it might be argued that the discrepancies are based upon the fallaciousness of this concept, rather than upon "negative interference", which is a summary re-description of the observations.

As to the question whether transient zygotes show the same aberrations as persistent diploids, I am convinced that they do. The most concrete evidence for this stems from some experiments of Gordon Allen's, who has succeeded in isolating nutritionally complementary recombinants from crosses of standard stocks on appropriately supplemented synthetic media: viz., MTL and  $PB_1$  from  $MP \times TLB_1$ . His data suggest that while the Lac segregations are complementary in these classes, as expected, the Mal segregations are not. However, you would learn more from a direct correspondence with him.

The following details may deserve your consideration:

a) The mapping of Mal, etc. The most puzzling finding has been that the addition of  $B_1$  (i.e. to recover  $B_1$ - as well as  $B_1^-$ ) has a negligible effect on the Mal segregation. Consequently,  $B_1$  would have to be located on a third "branch" from a point near M. Translocation?

b) Bibliography. Without having the papers with me to check, I still wonder whether a more appropriate reference instead of Lederberg 1950 would not be the little abstract in Genetics, 1950.

p2 Eosin-methylene blue might be more informative than EMB

p2 -3 Does the result that  $S^r \times S^d$  gives no  $S^S$  prove that these are simply alleles?  $S^d$  might be  $S^r$  plus some modifier.

p3 Discussion. Has crossing-over not been established?

p5 line 1 Lederberg, 1947.

References:

Lederberg 1950 recombinations, not recombination.

L & Tatum 1946 This is a <sup>4</sup>trivial paper. May I suggest substituting T&L, 1947 (J. Bact.) ?

Finally, I would suggest that the paper is considerably weakened by closing with a reference to Winkler. Aside from its inherent improbability, Konversion was already taken up, and disqualified, in 1947. A conclusion with the amendment sounds (subjectively) better.

Sincerely,

Joshua Lederberg  
Visiting Professor of Bacteriology \*  
(Associate Professor of Genetics, U. of Wis.)

\*P.S: at letterhead address July 31 - Sept. 6, 1950.

By the way, I am writing up a resume of bacterial genetics for an address at the Columbus meetings. May I refer to your so far unpublished results as embodied in the MSS you have so kindly sent me?